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Reducing Compliance Demands in Government Benefit Programs Improves the Psychological Well-Being of Target Group Members

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Abstract

State actions impact the lives of citizens in general and government benefit recipients in particular. However, little is known about whether experiences of psychological costs among benefit recipients can be relieved by reducing compliance demands in interactions with the state. Across three studies, we provide evidence that reducing demands causes relief. In a survey experiment, we show that psychological costs experienced by Danish unemployment insurance recipients change in response to information about actual reduced compliance demands. In two field studies, we exploit survey data collected around a sudden, exogenous shock (the COVID-19 lockdown of the Danish society in March 2020), which led to immediate reductions in compliance demands in Denmark's active labor market policies. We test whether two groups of benefit recipients experienced reduced psychological costs in response to these sudden reductions in compliance demands imposed by the state. Across all studies, we find that the reduction of compliance demands is associated with an increased sense of autonomy, and in two of the three studies, it is associated with reduced stress. Overall, our findings suggest that psychological costs experienced by benefit recipients are indeed affected by state actions in the form of compliance demands.

Abstract - Danish

Når borgere bliver mødt af krav fra det offentlige, kan det medføre oplevelser af psykologiske byrder i form af stress og en oplevelse af manglende selvbestemmelse over eget liv. På tværs af tre studier finder vi, at lettelsen af krav reducerer oplevelsen af psykologiske byrder. I et surveyeksperiment viser vi, at danske dagpengemodtageres oplevelse af belastning ændrer sig, når man informerer dem om faktiske lettelser af krav i dagpengesystemet. I to feltstudier bruger vi surveydata indsamlet før og under et pludseligt eksogent chok (nedlukningen af det danske samfund i forbindelse med COVID-19-pandemien i marts 2020), der medførte en midlertidig ophævelse af

We are grateful to 3FA for giving us the opportunity to collect survey data among their members and for inputs to the design of our surveys. All authors contributed equally to this research. The order of authors was randomized using the American Economic Association's randomization tool. See https://www.aeaweb.org/journals/policies/random-author-order/search?RandomAuthorsSearch%5Bsearch%5D=fTDlzzkwNY3n.

statens krav til modtagere af ledighedsydelser i Danmark. Her tester vi, om henholdsvis dagpenge- og kontanthjælpsmodtagere oplevede færre psykologiske byrder som følge af de reducerede krav. På tværs af alle tre studier finder vi, at lettelsen af krav er forbundet med en øget følelse af autonomi og i to af studierne finder vi ligeledes, at det er forbundet med mindre stress. Samlet set viser vores resultater, at ydelsesmodtageres oplevelse af psykologiske byrder er påvirket af de krav, som de møder i ydelsessystemet og altså ikke udelukkende er forårsaget af de livsomstændigheder, der giver anledning til behov for overførselsindkomster.

Introduction

State actions in the form of policy designs and implementation practices have been argued to cause experiences of psychological costs among affected citizens in the form of, for example, stress, stigma, and a sense of autonomy loss, especially when people participate in means-tested programs where they have to prove eligibility, fulfill activation requirements, or comply with restrictions (Christensen et al. 2020; Currie 2006; Deshpande and Li 2019; Herd and Moynihan 2019; Masood and Nisar 2020; Soss 1999).

Psychological costs matter. Not only to the people who experience them but also to the society as a whole. If state actions cause poor psychological well-being among target group members, it can potentially undermine program success as illustrated, for example, by job search literature showing that poor psychological well-being is negatively associated with job search and re-employment (Manroop and Richardson 2016; Paul and Moser 2009; van Hoye 2018). Moreover, psychologically burdensome state actions may harm fundamental democratic institutions by pushing target group members away from the rest of society. By signaling low societal standing and exclusion from valued social groups, burdensome state actions have the power to reduce trust in government, civic participation, and sense of civic duty among those affected (Bruch, Ferree, and Soss 2010; Kumlin 2004; Larsen 2019; Michener 2018). In effect, policymakers should care about psychological costs, not only out of compassion for target group members but also out of concern for the functioning of the society they are in charge of.

With few exceptions (e.g., Hattke, Hensel, and Kalucza 2020), the existing literature on psychological costs in citizen–state interactions is qualitative and focused on understanding people's experiences within a given system and the meaning they ascribe to these experiences (e.g., Barnes and Henly 2018; Masood and Nisar 2020; Nisar 2018). While valuable in their ability to provide deep and nuanced insights about their cases, such studies are less well equipped to evaluate the psychological impact of changes in compliance demands. In particular, these studies are rarely able to differentiate between the impact of compliance demands and the impact of interacting with the state or public programs *tout court*.

Patterns of poor psychological well-being among participants in means-tested programs with many compliance demands may easily emerge for reasons unrelated to the demands themselves (Campbell 2012; Mettler and Stonecash 2008). Conditions that cause the need to participate in means-tested programs, such as unemployment and related financial struggles, have their own negative psychological effects (Andreeva et al. 2015; Paul and Moser 2009; Wanberg 2012; Yoon et al. 2017). Consequently, a key methodological challenge is to disentangle effects of compliance demands from the effects of such other conditions. Yet, existing research has not taken up this challenge. We do so using survey-, quasi-, and natural-experimental research designs in the context of Denmark's social benefit and unemployment insurance systems (programs that are both targeted toward unemployed individuals).

First, we utilize a survey experiment to test whether unemployment insurance recipients' sense of autonomy, stress, and stigma are affected when we experimentally manipulate the saliency of a series of recent reductions in compliance demands in the benefit system. Second, drawing on two simultaneously deployed surveys, we exploit a sudden policy change that provides us with quasi- and natural-experimental variation in the compliance demands imposed on two samples of unemployed individuals (unemployment insurance recipients and social benefit recipients, respectively). In particular, we examine the impact of a sudden, exogenous reduction in compliance demands, which was introduced during our data collection as part of the COVID-19–related lockdown of the Danish society in March 2020. We estimate the effects of reduced compliance demands on recipients' experiences of stress, stigma, and sense of autonomy in relation to interacting with the two government benefit systems. Across studies, we find that respondents experience more autonomy in response to the reduced compliance demands. The effects on stress are more mixed, but generally point to lower levels of self-reported stress in response to the reduced demands. The evidence on balance does not support an effect of compliance demands on people's experiences of stigma.

The next section draws on psychological literature and public administration literature on administrative burden to develop our hypotheses about the effects of compliance demands on government benefit recipients'

experiences of psychological costs. This is followed by our presentation of the methods and analyses of each of our three empirical studies. Finally, we conclude with an overall discussion and by noting limitations and implications for theory and practice.

Theoretical Background

Administrative Burden in Citizen–State Interactions

Administrative burden can be defined as learning, compliance, and psychological costs associated with interacting with the state (Moynihan, Herd, and Harvey 2015). Learning costs arise when individuals have to acquire knowledge about the nature of, their eligibility for, and access to public services. Compliance costs arise when individuals need to comply with rules and requirements associated with state interactions, for example, filling in forms, traveling to participate in meetings with caseworkers, registering activities, or fulfilling behavioral requirements, such as mandatory work requirements, in order to access services and benefits. Finally, psychological costs may arise, for example, in the form of stress, stigma, or a sense of loss of autonomy because of experiences related to interactions with the state.

An important argument in the existing literature is that burdens are constructed (Herd and Moynihan 2019), meaning that the amount and nature of burden experienced in any given policy program to some extent is a function of state actions in the form of policy designs and implementation practices at the front line (Christensen et al. 2020, 128). Here, we use the term *compliance demands* to describe state actions that likely impose compliance costs, that is, state-imposed demands that target groups will have to comply with in order to take up services and benefits. Compliance demands often serve legitimate purposes and are not inherently bad (Madsen, Mikkelsen, and Moynihan Forthcoming). However, they do impose experiences of administrative burden on those who are affected, and to fully assess the effects (including benefits *and* costs) of how policies are designed and implemented, these experiences should be taken into account.

The term compliance demands does not exclude the possibility that state actions that impose learning costs may also contribute to psychological costs among target groups. In fact, many of our arguments are agnostic in the short term as to whether compliance demands are associated with acquiring knowledge about or operating in a demanding administrative system. However, as we argue, our empirical investigation centers on compliance costs and, additionally, gives little indication that effects are transient, as one would intuitively expect were they based solely or mainly on learning costs.

Compliance Demands and Experiences of Psychological Costs

When strict compliance demands are imposed on citizens, there are a variety of mechanisms through which the demands may cause experiences of psychological costs, depending on how they are designed and experienced. Although our data does not allow us to disentangle these mechanisms empirically, we discuss them as a basis for our hypotheses.

First, if compliance demands are coupled with sanctions, such as suspensions of or even disentitlement from benefits in case of noncompliance, it may lead to uncertainty and worries about the consequences, should one fail to understand or live up to the demands. For example, to remain eligible for their benefits, the respondents in our investigation (Danish unemployment benefit recipients) are required to apply for multiple jobs each week and to document their job search activities using an online platform before the end of each week. In effect, people are never more than a few days from the next deadline with a potential (if missed) to cause serious financial problems. The psychological impact of this continuous pressure may even be bigger among participants in means-tested programs than among other people. Conditions that cause people to need assistance from these programs (e.g., unemployment and poverty) do often deplete cognitive resources needed to "plan activities ahead of time, act on those plans, and stay on task despite impulses and temptations to do something else when things get frustrating" (Christensen et al. 2020, 130), thereby making it harder to manage compliance demands and increasing the risk of missing administrative deadlines.

Second, compliance demands "bring practical meaning to abstract political concepts, such as rights and obligations, power and authority, voice and civic standing" (Soss, Fording, and Schram 2011, 284). If participants in benefit programs are forced to live with constraining and stigmatizing compliance demands, it may convey to them that, in the eyes of the society, they "do not deserve the autonomy that other citizens have in living their lives and cannot be trusted with government resources" (Herd and Moynihan 2019, 156). Such messages of distrust and devaluation are psychologically painful as humans are extremely sensitive to cues of (dis)approval from their surroundings. From an evolutionary perspective, concerns about other people's approval have been adaptive as "gaining social approval, social acceptance and support offered considerable advantages over the course of human evolution" (Gilbert 2000, 175). In effect, as humans, we have evolved a fundamental need to belong (Baumeister and Leary 1995), causing us to "seek to be *chosen* as a friend or lover; to be invited to join the team and so forth" (Gilbert 2000, 175) and when we are excluded by others or our social value is being put into question, it tends to generate negative psychological reactions such as stress (Major

and O'Brien 2005), anxiety (Major and O'Brien 2005; Williams 2001), reduced self-esteem (Crocker 1999), and shame (Gilbert 2000; Sznycer et al. 2018), even if we do not think we are guilty of any wrongdoing (Robertson et al. 2018).

Third, compliance demands restrict people in their actions and limit their freedom to decide how to manage their life, meaning that they threaten people's need for autonomy. The need for autonomy, which takes center stage in theories of basic psychological needs (Deci and Ryan 2000; Vansteenkiste, Ryan, and Soenens 2020), means that human thriving requires us to be self-governed in our choices. Thus, evidence shows that it is psychologically taxing to engage in activities, that are mandatory or have been forced by external forces (Reis et al. 2000).

The idea that compliance demands have psychological consequences is central in policy feedback literature where the lived experiences of participants in policy programs serve as a mechanism through which policies can affect target groups' political views and levels of civic participation (for good introductions to this literature, see Campbell 2012; Larsen 2019; Mettler and Soss 2004). Most investigations of the psychological consequences of compliance demands have been based on qualitative case studies. For example, some studies have pointed to experiences of *stress* resulting from documentation requirements. Requirements are especially stressful when people do not recognize a clear and legitimate purpose behind them (Caswell, Larsen, and Sieling-Monas 2015; Hasluck and Green 2007), meaning that they find themselves "stagnated" (Danneris 2018; Danneris and Nielsen 2018; Müller et al. 2010) in a meaningless administrative system unresponsive to their individual needs. Other studies have focused on how *stigma* follows when heavy compliance demands, such as frequent compulsory case worker meetings or activation courses, are perceived as messages of social devaluation (Dias and Maynard-Moody 2007; Manchester and Mumford 2010; Sage 2018; Soss 1999, 2005; Stuber and Schlesinger 2006) which may even discourage eligible citizens from applying for assistance in the first place (Auyero 2011; Bartlett, Burstein, and Hamilton 2004; Stuber and Schlesinger 2006). Finally, research has documented that benefit recipients often experience a *loss of autonomy*, for example, because they are restricted in or feel they have to justify certain actions in order to maintain eligibility for benefits (Bisgaard 2018; Lipsky 1980; Mik-Meyer 2017; Soss 1999).

In addition to such qualitative studies, a growing body of quantitative (including field and quasi-experimental) research has investigated effects of compliance demands on people's take-up of government services and benefits. Studies have found compliance demands to be associated with limited take-up, especially among the least advantaged (i.e., most needy) groups of individuals (Bhargava and Manoli 2015; Brodkin and Majmundar 2010; Christensen et al. 2020; Currie 2006; Deshpande and Li 2019; Homonoff and Somerville 2020). However, while psychological costs may be one mechanism linking compliance demands to reduced take-up, the relationship between compliance demands and psychological costs has not received much empirical attention in this research tradition.

Drawing on existing theorizing and findings, we investigate effects of compliance demands on three kinds of psychological costs: target group experiences of (1) stress, (2) stigma, and (3) autonomy loss associated with receiving government benefits. This is not an exhaustive list of psychological costs that may emerge in response to compliance demands (examples of other negative psychological reactions could be depressive or anxious thoughts, lack of motivation, abusive behavior, excessive drinking, etc.) but stress, stigma, and autonomy loss are the ones that have been highlighted in early definitions of psychological costs (Moynihan, Herd, and Harvey 2015) and have been examined in the public administration literature. This makes them relevant as a starting point for our investigation. Thus, we test the following three hypotheses:

H1: Reductions of compliance demands will cause government benefit recipients to feel less stress in relation to their interactions with the benefit system.

H2: Reductions of compliance demands will cause government benefit recipients to feel less stigma in relation to receiving government benefits.

H3: Reductions of compliance demands will cause government benefit recipients to feel more autonomous over their lives.

Current Challenges Addressed in Our Studies

Although the existing literature has made a strong case that relationships exist between compliance demands in government benefit systems and experiences of psychological costs among benefit recipients, it is still an open question whether these relationships are causal. As Campbell (2012, 343) notes, it is far from random who gets to participate in different programs. For example, means-tested programs, where the eligibility of participants has to be frequently established and documented, often involve more and stricter compliance demands than universal programs (Campbell 2012; Mettler and Stonecash 2008). Moreover, many of the conditions that leave people in need of means-tested programs (e.g., unemployment and poverty) have negative psychological effects themselves (Andreeva et al. 2015; Paul and Moser 2009; Wanberg 2012; Yoon et al. 2017). This means that empirical research is vulnerable to conflating effects of compliance demands

with effects of the conditions that brought people into contact with public programs to begin with.

One laboratory experiment sheds light on people's emotional responses to burdensome policy designs by testing the causal effects of an experimental treatment employing a burdensomely designed form to be filled in by respondents (Hattke, Hensel, and Kalucza 2020). However, the high internal validity obtained in the lab comes at a price because the artificial lab setting is, everything else equal, associated with other challenges, especially regarding ecological validity (Morton and Williams 2010). There is thus a need for studies designed to disentangle the effects of compliance demands from potentially confounding factors in ecologically valid settings.

In addition to the unanswered questions about causality, it is not straightforward that changes in compliance demands can reduce psychological costs among existing users of the system, at least in the short run, even if a causal relationship exists. As noted by Herd and Moynihan (2019), "the incidence of psychological costs [are not] one-off negative experiences arising from discrete interactions. [Instead,] (…) psychological costs accumulate and last, generating lifelong stresses in how administrative burdens are experienced" (152–3). Likewise, Campbell (2012) argues that when policy designs and implementation practices change, "attitudinal change may only come in the long term with generational replacement, rather than through change within individuals" (344).

The summarized gaps in current knowledge are problematic from the perspectives of both research and practice because it is unclear how much direct influence policymakers and other state actors have on the psychological well-being of target group members. There is therefore a need for evidence about the causal effects, including in the short term, of compliance demands on experiences of psychological costs. The existing literature renders it probable that compliance demands matter to people's experiences of psychological costs but stops short of providing strong, causally interpretable evidence produced in the field. Our studies address this shortcoming.

Empirical Background

The empirical setting of our investigation is the Danish unemployment benefit system. Underpinning the Danish *flexicurity* model, which combines the flexibility of eased employment protection legislation with the security of a strong social safety net, unemployed recipients of government benefits are offered generous allowances in return for multiple obligatory activation requirements and sanctioning of noncompliant behavior (Andersen 2019). The two overarching recipient categories, *unemployment insurance recipients* and *social benefit recipients*, differ in their insurance status prior to entering unemployment and the size of the benefits varies accordingly.[1] However, both groups are subject to strict active labor market policies involving a number of compliance demands. Demands include requirements to apply for multiple jobs each week, frequent documentation and registration of employment-related activities, frequent meeting attendance with caseworkers, compulsory job training, and an obligation to accept even job offers that do not match the beneficiary's educational or professional background.

In our investigation, we rely on data collected among unemployment insurance recipients and social benefit recipients right before and during the COVID-19-related lockdown of the Danish society in the spring of 2020. The lockdown, which commenced after a surprise announcement by the country's Prime Minister on the evening of March 11, 2020, caused an immediate suspension of all compliance demands in the Danish unemployment system starting the morning of March 12, meaning that from one day to the next, the strict active labor market policies were replaced by a *completely unconditional* system—a change that lasted until May 27, 2020 (STAR 2020).

In Study 1, which we carried out among unemployment insurance recipients in April 2020, we investigate psychological reactions to an experimental manipulation of the salience of the recent reductions of compliance demands in the system. In Studies 2 and 3, which we carried out among a sample of unemployment insurance recipients and a sample of social benefit recipients in February and March 2020, we utilize the sudden, exogenous nature of the suspension of compliance demands combined with the timing of our data collection right before and after the announcement of the suspensions of demands, meaning that we have quasi- (Study 2) and natural- (Study 3) experimental variation in compliance demands. Key information about the three studies are summarized in table 1.

Study 1: Survey Experiment Manipulating the Salience of Compliance Demands among Unemployment Insurance Recipients

Participants

Study 1 is based on a survey sent to 5,000 unemployment insurance recipients who were all members of the

1 In addition, social benefit recipients in Denmark are subcategorized into two groups depending on their age and ability to work. The social benefit recipients surveyed for this study are "job ready" social benefit recipients who face no conditions other than unemployment and are subject to similar requirements as unemployment insurance recipients. This includes individuals above the age of 30 and individuals below the age of 30 with a qualifying secondary education or above. Individuals facing additional conditions, such as health or social challenges, are categorized as "activation ready" social benefit recipients and subject to a different range of requirements depending on their age and educational background. This latter group was not surveyed for the study.

Table 1. Overview of Studies

	Benefit Program	Type of Data	Time of Data Collection	Sample Size
Study 1	Unemployment insurance	Survey experiment (random assignment)	April 7–17, 2020	996
Study 2	Unemployment insurance	Quasi experiment (as-if random assignment)	March 9–31, 2020	2,614
Study 3	Social benefits	Natural experiment (random assignment)	February 23–March 28, 2020	784

Danish unemployment insurance fund 3FA, which primarily serves blue-collar workers. Using simple random sampling, the respondents were selected from the fund's complete list of unemployed members at the time of the data collection. The data was collected from April 7 to 17, 2020, and consists of 996 completed or partially completed responses (a response rate of 20%). While older and female respondents were somewhat overrepresented, the completed response sample roughly corresponds to the target population on characteristics recorded in 3FA's administrative records. The average age of respondents was 50 years (44 in the population) and 36.7% were women (30.4% in the population).[2] See supplementary appendix B for further descriptive information on the sample.

Experimental Procedure

To provide causal evidence of the effects of compliance demands on target group experiences of psychological costs, we need a research design which is tailored to overcome the endogeneity problems associated with observational studies, that is, the risk of results being biased by omitted variables and reverse causality. To overcome this challenge, Study 1 utilizes the high internal validity of a randomized survey experiment. With the ability to both design and randomize the allocation of treatment material in the experiment, this method allows us to control our independent variable and effectively keep (the salience of) compliance demands exogenous to experiences of psychological costs.

The study's experimental material was designed to manipulate the salience of the suspended compliance demands in the unemployment insurance system, which had recently been announced when the data was collected. The treatment material consisted of a short number of factual questions about the sudden unconditionality of the system as well as a text summarizing the specific reductions in compliance demands that were introduced.[3] Respondents were randomly assigned to either a treatment group (which was presented with the treatment material before answering the items measuring our dependent variables) or a control group (which saw the experimental material only at the end of the survey after the items used to measure the dependent variables). In our analyses, people who were aware of the reduced compliance demands in advance (i.e., those who provided correct answers to the factual questions in the beginning of the treatment material) are included on an equal footing with people who were not aware of these reductions, meaning that the estimated treatment effects are somewhat conservative. A balance test (reported in supplementary appendix C) shows no imbalances between the two experimental groups on any of our observed background variables.

Measures

To probe our hypotheses, we test for treatment effects on respondents' experiences of stress, stigma, and autonomy loss related to receiving unemployment insurance benefits. To measure these three kinds of psychological costs, we rely on items that have previously been used in research by Thomsen, Baekgaard, and Jensen (2020), adapted to the context of our investigation.

The items were placed shortly after the treatment. Three items measure experiences of stress, three items measure experiences of stigma, and four items measure autonomy loss. All items were measured on a 5-point Likert scale ranging from "completely disagree" to "completely agree." The wording of all items is shown in table 2. Based on the items, three scales were created, all showing high internal consistency. Coefficient omega was above conventional benchmarks in all scales (ω = 0.77 for autonomy loss; 0.85 for stress; 0.90 for stigma).[4] In a confirmative factor analysis, estimated using full information maximum likelihood (FIML), a three-factor measurement model fits the data well (Model Chi-Square [χ^2] = 110.354 [degrees of freedom [df] = 32, $p < .001$], comparative fit index [CFI] = 0.985, root mean square error of approximation [RMSEA] = 0.051, standardized root mean square residual [SRMR] = 0.023) and significantly better than

2 Our findings are robust to a specification controlling for both age, gender, and a series of other variables, suggesting that the slight skewness of our sample does not substantially affect our estimates (supplementary appendix E).

3 For original (Danish) wording of the experimental material, see supplementary appendix A.

4 We report coefficient omega as opposed to Cronbach's alpha as the latter has been criticized for misrepresenting reliability, for example, when its assumption of tau equivalence is violated. Coefficient omega addresses this issue as it permits congeneric measurement. Alpha often underestimates construct reliability. However, as we show in supplementary appendix P, all scales in all studies show alpha values above conventional benchmarks.

Table 2. Item Wordings

Item Wording	Studies 1	2	3
Stress			
1."I have often been in a bad mood since becoming an unemployment insurance benefit recipient" / "Being on social benefits puts me in a bad mood"	X	X	X
2."I have slept less well at night since becoming an unemployment insurance benefit recipient" / "I sleep poorly at night"	X	X	X
3."I am often stressed and nervous because of receiving unemployment insurance benefits" / "Being on social benefits stresses me and is making me nervous"	X	X	X
4."I feel in good spirits" (reversed)			X
Stigma			
5."When I meet new people, I prefer to hide that I'm on [unemployment insurance/social benefits]"	X	X	X
6."I feel frowned upon because of receiving [unemployment insurance/ social benefits]"	X	X	X
7."I am afraid of being looked down upon because of receiving [unemployment insurance/social benefits]"	X	X	X
8."Those around me respect me for who I am" (reversed)			X
Autonomy loss			
9."I feel forced to do things that I do not want because of receiving unemployment insurance benefits" / "I often feel forced to do things that I do not want to do because of receiving social benefits"	X	X	X
10."I feel that I have influence over the processes related to receiving [unemployment insurance/social benefits]" (reversed)	X	X	X
11."As an [unemployment insurance/social benefit] recipient, I cannot organize my everyday life as I want to"	X	X	X
12."As an [unemployment insurance/social benefit] recipient, I do not feel in control over my own life"	X	X	X

Note: The table shows item wordings used in the three studies, translated from Danish to English. For original Danish wording, see supplementary appendix A and F.

a single factor model ($\Delta\chi^2$ = 578.859 [df = 3, p < .001], ΔCFI = 0.108, ΔRMSEA = 0.091, ΔSRMR = 0.031) (supplementary appendix D). In our subsequent analyses, we use factor scores from this model as our dependent variables. To make estimates easily interpretable, we rely on effects coding for model identification, implying that our scales for stress, stigma, and autonomy loss use the same 5-point scale as their indicators.

Results

We use randomization inference with calculations of p values based on 5,000 resamples to estimate the average treatment effects (ATE) on each of the three indexes described above (measuring experiences of autonomy loss, stress, and stigma, respectively).[5] For identification, we mean center and standardize our dependent variables.

Figure 1 shows our results. We find a significant and negative ATE of our treatment on stress, as suggested by H1 (Estimate [Est] = –0.127, standard error [SE] = 0.061, p = .036). Similarly, consistent with H2, we find the expected negative ATE on stigma, albeit significant only at the 10% level (Est = –0.119, SE = 0.062, p = .059). Finally, as suggested by H3, we find the negative ATE on autonomy loss (Est = –0.112, SE = 0.044, p = .012).

Study 1 lends support to our hypotheses as all three dimensions of psychological costs are affected in the expected direction in response to our manipulation although the results are somewhat weaker for stigma than for stress and autonomy loss. However, although our survey experiment provides comparatively strong evidence in terms of internal validity, it is not without limitations. First, the experiment manipulates the *salience* of (suspended) compliance demands, not compliance demands per se. While using such priming effects to study difficult-to-manipulate variables is commonplace, finding effects of boosting the salience in a survey does not automatically imply that the same effect occurs outside the experimental setting (Morton and Williams 2010). Moreover, survey experiments involve a somewhat artificial setting for respondents, and even effects of using the exact same information

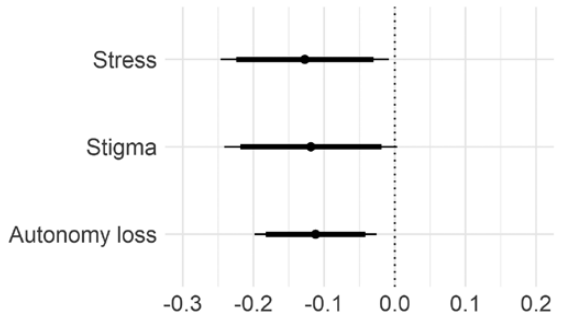

Figure 1. Main Results of Study 1

Note: Difference in means estimates for Study 1 with bootstrapped 95% and 90% (bolded) confidence intervals.

5 Due to the use of randomization inference, the results of the ATE contain slightly different p values and standard errors than the corresponding results of regression models without any control variables.

may not replicate if studied in the field (Barabas and Jerit 2010).

For these reasons, Study 2 tests our hypotheses using field data collected among unemployment insurance recipients right before and during the beginning of the period where compliance demands were suspended due to the COVID-19 lockdown of the Danish society. As we detail below, the internal validity of this approach is more questionable than that of Study 1. However, it allows us to examine the effects of *real changes of compliance demands* among respondents similar to those having participated in our experiment, thereby ensuring more ecologically valid findings than in Study 1.

Study 2: Quasi Experiment among Unemployment Insurance Recipients

Participants

Like Study 1, Study 2 draws on data collected among unemployed members of the unemployment insurance fund 3FA. For this study, the survey was sent to 10,000 unemployed members of the fund, again selected from their full list of unemployed members using simple random sampling. The data were collected between March 9 (i.e., 3 days prior to the announcement of the decision to suspend compliance demands in the employment system) and March 30, 2020, while the suspension of active labor market policy requirements was still in place. The timing of the data collection allows us to utilize the sudden shock in compliance demands as a quasi-experimental "treatment," meaning that we can investigate the psychological impact of the suspended compliance demands by comparing answers from before and after the suspension. We discuss the feasibility of this strategy below.

We gathered 1,041 completed or partially completed responses prior to and 1,573 after the suspension of the compliance demands. Though women were again somewhat overrepresented, the sample of respondents roughly corresponds to the information available about our target population. The average age of respondents was 48 years (52 in the population) and 38.8% were women (31.2% in the population). See supplementary appendix G for further information about the sample.

Measures

The study used the same measures for stress, stigma, and autonomy loss as Study 1. Supplementary appendix G shows the descriptive statistics for each of the measures. As in Study 1, all three scales show high internal consistency. Coefficient omega was above conventional benchmarks for all scales ($\omega = 0.74$ for autonomy loss; 0.86 for stress; 0.90 for stigma), and again, a three-factor measurement model fits the data well ($\chi^2 = 169.937$ [df = 32, $p < .001$], CFI = 0.989, RMSEA = 0.044, SRMR = 0.028) and significantly better than a single factor model ($\Delta\chi^2 = 2118.711$ [df = 3, $p < .001$], ΔCFI = 0.175, ΔRMSEA = 0.127, ΔSRMR = 0.045) (see supplementary appendix H for further details). As in Study 1, we utilize effects coded factor scores from the three-factor model for our analyses.

Owing to its quasi-experimental design, the study controls for gender, age, education, length of unemployment, number of unemployment periods in the last 5 years, and number of children. Gender and age are drawn from the administrative registries of 3FA. Education was measured in the survey asking respondents to select their highest level of completed education from a list of common options from primary school to university levels of education. Number of children, unemployment periods, and length of unemployment, too, were self-reported in the survey (for details, see supplementary appendix I).

Results

To analyze the effects of the suspension of compliance demands in the unemployment insurance system, we utilize the announcement of the suspension of compliance demands in the benefit system on the morning of March 12 as a discontinuity in burdens imposed on unemployment insurance recipients.[6] To some extent, then, the comparison of psychological costs before and after the announcement can be considered a comparison of control and treatment groups in a regression discontinuity design (RDD). Particularly for observations clustered closely around a discontinuity, comparisons can arguably leverage as-if random assignment relative to the discontinuity (Dunning 2012).

Our design focuses on a discontinuity in time, which often makes the assumption about as-if random assignment somewhat questionable (Hausman and Rapson 2018), which is also reflected in the balance test in supplementary appendix J showing that fewer parents ($p < .001$) and fewer women ($p = .083$) answered the survey before the policy change on March 12 compared to after the change. To bias estimates, respondents' decisions to respond prior to rather than after March 12 would have to either be affected by the expectation that compliance demands would be suspended or share a common cause with the experience of psychological costs. We consider the former unlikely. Denmark was the second country in Europe (after Italy) to initiate a societal lockdown, and the suspension of compliance demands—which was a direct consequence of the

6 We constructed our treatment variable using respondents' start time for the survey since using end time excludes respondents who did not finalize the survey. Using end times rather than start times reduces our number of observations but does not qualitatively alter our conclusions.

lockdown decision—was unlikely to be anticipated by respondents. However, potential common cause confounding cannot be ruled out entirely. For example, it would be problematic if people who experience the unemployment insurance system as very psychologically burdensome are more motivated to participate in surveys about the system (as a way to voice their grievances) than less burdened individuals, causing them to be quicker to respond and thereby creating a bias in the timing of participation.

For this reason, while we adopt the logic of an RDD, some caution is needed when making causal claims based on Study 2. As a robustness check, we estimate the effects of the policy suspension including the series of control variables listed in the measures section above. As in Study 1, we predict factor scores for our three dependent variables and standardize and means center the factors in all analyses (supplementary appendix H).

First, we report simple difference-in-means results using randomization inference with 5,000 resamples to calculate p values. The results, shown graphically in figure 2, are consistent with our hypotheses regarding two of our three measures of psychological costs. Consistent with Hypotheses 1 and 3, we observe the expected differences between respondents answering before versus after the policy change when investigating changes in stress (Est = –0.080, SE = 0.040, p = .044) and autonomy loss (Est = –0.093, SE = 0.028, p = .001). Respondents participating in the survey before the policy change on the morning of March 12 were, on average, more stressed and experienced less autonomy over their lives than respondents participating after the policy change. However, Hypothesis 2 does not find support in this study as we do not find a similar change in experienced stigma (Est = 0.018, SE = 0.041, p = .658).

The size of the change estimates for stress and autonomy loss resemble the ATE estimates from Study 1. The treatment effect on stigma in Study 1 (which was, however, only significant at the p < .1 level) is not found in Study 2. Unlike Study 1, which relied on a randomized experiment, the estimates in Study 2 are vulnerable to common cause confounding, particularly due to the quasi-experimental nature of the design. As demonstrated in table 3, however, the results are robust to the inclusion of control variables in an ordinary least square regression analysis, and differences in estimates between models with and without controls are minor in all cases. Thus, the p values for the differences between announcement estimates from models with and without controls included are 0.877, 0.723, and 0.921 for stress, stigma, and autonomy loss, respectively. This suggests that the quasi-experimental treatment is in fact an exogenous predictor of psychological costs.

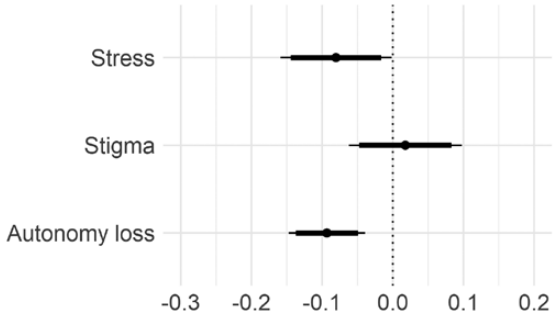

Figure 2. Main Result of Study 2

Note: Difference in means estimates for Study 2 with bootstrapped 95% and 90% (bolded) confidence intervals.

Although the results above are more mixed than in Study 1, Study 2's overall pattern of reduced experiences of stress and an increased sense of autonomy following the suspension of compliance demands is important as it increases our confidence that compliance demands affect experiences of psychological costs outside the artificial setting of the survey experiment reported in Study 1. However, although the results are similar to the survey experimental results in Study 1, Study 2's internal validity is challenged by our inability to reject the risk of common cause confounding, meaning that caution is needed when making causal claims based on this study. Moreover, Studies 1 and 2 both rely on answers from one rather homogenous group of government benefit recipients: unemployed blue-collar workers who were all insured prior to entering unemployment, meaning that we do not know if the results generalize to other groups of benefit recipients. In light of these limitations, we add Study 3 to our investigation.

Study 3: Natural Experiment among Social Benefit Recipients

Participants

In addition to improving causal identification, a main goal of Study 3 is to assess the generalizability of our findings across different groups of government benefit recipients. To do so, we rely on data collected among another group of benefit recipients than those participating in Studies 1 and 2: social benefit recipients. Social benefits is a government benefit program targeted toward unemployed individuals who were uninsured prior to entering unemployment or who have lost their eligibility for unemployment insurance benefits. Social benefit recipients tend to have fewer financial resources prior to becoming beneficiaries as eligibility presupposes that people's assets do not

Table 3. Regression Results for Study 2

	Stress		Stigma		Autonomy loss	
After announcement	−0.080 (.044)	−0.089 (.037)	0.018 (.663)	0.039 (.374)	−0.093 (.001)	−0.097 (.001)
Gender: male		−0.157 (.000)		−0.122 (.006)		−0.096 (.002)
Age		−0.002 (.256)		−0.008 (.000)		0.000 (.910)
Education: secondary		−0.004 (.937)		−0.022 (.653)		0.000 (.998)
Education: tertiary		−0.000 (.995)		0.088(.222)		0.007 (.888)
Unemployment status: other		−0.063 (.189)		−0.079 (.105)		−0.028 (.408)
Unemployment length: 6–10 weeks		−0.046 (.541)		−0.105 (.172)		−0.032 (.540)
Unemployment length: 11–15 weeks		−0.070 (.336)		−0.095 (.205)		−0.041 (.420)
Unemployment length: 16–20 weeks		0.094 (.234)		−0.017 (.835)		0.065 (.235)
Unemployment length: 21 or more weeks		0.223 (.000)		0.163 (.012)		0.174 (.000)
Number of unemployment periods		0.011 (.462)		−0.003 (.844)		0.017 (.111)
Number of children: one child		0.155 (.012)		0.117 (.064)		0.105 (.014)
Number of children: two children		0.172 (.030)		0.118 (.147)		0.106 (.054)
Number of children: three or more children		0.240 (.048)		0.332 (.007)		0.186 (.027)
(Intercept)	2.172 (.000)	2.246 (.000)	1.634 (.000)	2.070 (.000)	2.203 (.000)	2.127 (.000)
N	2,207	1,888	2,207	1,888	2,207	1,888
R-squared (adj.)	.002	.036	.000	.041	.005	.039

Note: Entries are ordinary least squares regression coefficients. *p* Values in parentheses.

exceed 10,000 Danish Kroner (around 1,600 USD).[7] Furthermore, the level of social benefits is markedly lower than that of unemployment insurance and social benefit recipients are more often in contact with the unemployment system for longer periods of time. As we noted in the "Empirical Background" section, social benefit recipients are subject to compliance demands that are similar to those in the unemployment insurance system but on March 12, because of the COVID-19 lockdown of the Danish society, the compliance demands of the system were completely suspended. Like in Study 2, Study 3's data was collected shortly before and after the announcement of the suspended demands.

Respondents for Study 3 were selected from a complete list of all social benefit recipients deemed "job ready" in Denmark. A total of 5,704 respondents were selected using simple random sampling. Unlike Study 2, however, respondents in Study 3 were randomized (ahead of the announcement regarding policy changes) into one of three groups varying only in the timing of survey invitations being sent out. For each respondent, simple randomization was used to decide whether invitations were sent on February 23, February 29 (both dates before the policy change was announced), or March 14 (2 days after the policy change took effect). Because of the random assignment to survey invitations, Study 3's results do not share Study 2's risk of endogeneity in terms of who answered the survey before versus after the announcement of the suspended compliance demands, meaning that we have a much cleaner natural experiment than in Study 2 (Dunning 2012). We gathered 508 responses prior to and 276 after the policy change.[8] A balance test (reported in supplementary appendix L) does not show any differences in sociodemographic profiles between respondents answering the survey before compared to after the change on March 12.

Measures

With few exceptions, Study 3's measures of respondent experiences of stress, stigma, and autonomy loss are largely identical to the measures used in Studies 1 and 2, although items had to be slightly adapted to make sense in the context of the social benefit system (for an overview of similarities and differences between measures, see table 2. For original Danish item wording, see supplementary appendix F). As in Studies 1 and 2, all three scales show high internal consistency. Coefficient omega was above conventional benchmarks for autonomy loss ($\omega = 0.75$), stress ($\omega = 0.80$), and stigma ($\omega = 0.82$). A three-factor measurement model fits the data decently, although not as well as in Studies 1 and 2 ($\chi^2 = 201.229$ [df = 51, $p < .001$], CFI = 0.952, RMSEA = 0.063, SRMR = 0.038) but still significantly better than a single factor model ($\Delta\chi^2 = 360.843$ [df = 3, $p < .001$], ΔCFI = 0.146, ΔRMSEA = 0.080, ΔSRMR = 0.038) (see supplementary appendix M

7 If applicants live in a romantic relationship, the partner's assets are accounted for as well. Thus, to be eligible for social benefits, the combined assets of the applicant and their partner cannot exceed 20,000 Danish Kroner.

8 Twenty-three respondents were removed from the analysis because they were invited to respond to the survey prior to, but in fact responded after, the policy change announcement. Treating these respondents as non-compliers we obtain CACE estimates which qualitatively sustain our conclusion from the main text that the lockdown is associated with less stress (Est = −0.121, SE = 0.058, $p = .038$) and less autonomy loss (Est = −0.128, SE = 0.050, $p = .011$) but not significantly associated with stigma (Est = −0.037, SE = 0.060, $p = .536$).

for factor loadings).[9] As in Studies 1 and 2, we utilize effects-coded factor scores from the three-factor model for our analyses.

Results

Figure 3 shows that the suspension of compliance demands led Study 3's respondents to feel less stressed by the social benefit system, which is consistent with Hypothesis 1 (Est = −0.121, SE = 0.058, p = .035) (based on using randomization inference with 5,000 resamples to calculate p values). Moreover, consistent with Hypothesis 3, respondents experienced an increased sense of autonomy over their lives in response to the changes in the system (Est = −0.128, SE = 0.050, p = .010). Like in Study 2, our results do not support Hypothesis 2 as we do not find any effect on experienced stigma associated with receiving social benefits (Est = −0.037, SE = 0.061, p = .544).[10]

Discussion

Our studies provide survey-, quasi-, and natural-experimental evidence of causal effects of compliance demands on psychological costs experienced by recipients of two kinds of unemployment benefits in Denmark. Across all studies, we find the reduction of compliance demands to be associated with an increased sense of autonomy, and in two of the three studies, it is associated with reduced stress.

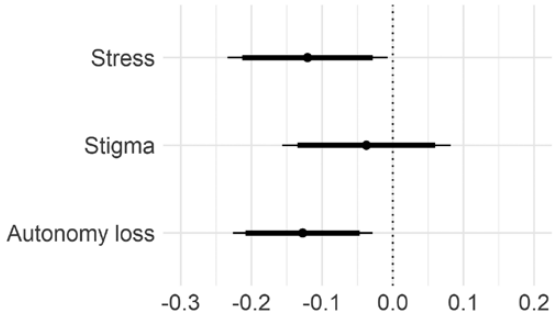

Figure 3. Main Results of Study 3

Note: Difference in means estimates for Study 1 with bootstrapped 95% and 90% (bolded) confidence intervals.

The effects of the reductions should not be exaggerated, however. In all studies, Cohen's D for statistically significant findings (p < .05) are spanning between 0.12 and 0.21, and the effect sizes are thus very modest. How can this be? Why do we not find stronger effects? One likely reason is that much stress, stigma, and sense of autonomy loss is associated more with being a benefit recipient (and the uncertainty it entails with regard to individual finances and the future) than it is with compliance demands as such. Also, there are limits to how responsive we can expect psychological costs to be. Psychological states like stress and sense of autonomy loss can either be episodic or chronic. For people who are in a permanent state of stress, the reduction of compliance demands is likely to have little or no effect at all.

Also, we note some limitations of our studies. First, the effects in our studies are short-term effects of a large but temporary policy change, meaning that we are limited in our ability to predict the long-term effects of more permanent reductions of compliance demands. On the one hand, permanent policy changes are more fundamental than temporary changes and thus, permanent changes constitute a stronger "treatment," suggesting that effects would be even stronger than the effects of the sunset-provisioned policy change that we studied in our investigation. However, it is also possible that some of the effects would fade out over time, as people get used to the new normal. For instance, according to the expectation-disconfirmation theory of citizen satisfaction (Van Ryzin 2004), people react positively when their experiences of the state are better than they expected. Also, people's expectations may in part be a product of their prior experiences with the state (Hjortskov 2019). In effect, it is possible that our findings to some extent are results of people reacting to the unemployment benefit system becoming less burdensome than they had come to expect based on their prior experiences in the system, while the long-term effects would be weaker due to revised and more positive expectations. More research is therefore needed to shed light on the long-term effects of more permanent changes in compliance demands.

Moreover, the reduction of compliance demands, which we utilized in our investigation, involved a complete suspension of all the (usually strict) active labor market policies of the unemployment system, meaning that the benefits became completely unconditional from one day to the next. The fact that our quasi- and natural-experimental treatment consists of a package of changes makes us unable to identify the effects of each individual element of the package: Did people react to the suspended job search and

9 A measurement model replicating as closely as possible items from Studies 1 and 2, and thus omitting two items, gives a somewhat improved fit (χ^2 = 91.856 [df = 32, p < .001], CFI = 0.978, RMSEA = 0.050, SRMR = 0.031).

10 For the sake of completeness, supplementary appendix N presents results from models using a set of control variables—measured as indicated in supplementary appendix I, in an OLS framework. These models sustain our results. Moreover, similar to Study 2, the introduction of control variables does little to change the estimates. In addition, we re-estimated our models using only the subset of items available across all our studies. As shown in supplementary appendix O, this model sustains our conclusions, although the results for stress are somewhat suggestive.

documentation requirements? To not having to meet with caseworkers? Or to the suspension of sanctions in the system? Also, under normal circumstances, it is hard to imagine changes in compliance demands that are as sudden and dramatic as those utilized in our investigation. The sudden nature of the changes allows us to obtain evidence with a high level of internal validity, but some caution is needed in terms of predicting effects of reforms that utilize narrower tools or are less dramatic. Thus, we encourage research on the effects of separate compliance demands and less extreme interventions.

Furthermore, we have to consider the fact that the suspension of compliance demands in our study was announced in the context of an extraordinary situation: the national lockdown of Denmark in response to the COVID-19 pandemic during the spring of 2020. After all, the entire country was in a state of emergency, and research has documented that people reacted by "rallying around the flag," increasing their trust in government (Baekgaard et al. 2020; Bol et al. 2020). Could the reduced psychological costs, which we saw in our studies, be part of people's psychological reaction to this more general state of emergency, instead of a reaction to the suspension of compliance demands in the unemployment benefit system? Are we conflating the effects of compliance demands with effects of people's gratefulness for being alive and healthy in the midst of a global pandemic?

There are at least three reasons to believe that this is not the case. First, the conclusions from our quasi- and natural-experimental studies are consistent with the results of our survey experiment (Study 1). That study was carried out in April 2020, more than 3 weeks after the beginning of the lockdown, and thus, *if* the lockdown affected people's experiences of psychological costs (independent of the effects of the suspended compliance demands), all respondents were subject to this influence.

Second, while we cannot reject the possibility of Studies 2 and 3's results being affected by the state of emergency associated with the pandemic-related lockdown, we find this to be an unlikely explanation of the improved psychological well-being that we observed among our respondents. The existing literature on the psychological effects of epidemics-related quarantines has pointed to severe negative effects "including post-traumatic stress symptoms, confusion, and anger" (Brooks et al. 2020). In a study of the psychological impact of the Danish COVID-19 lockdown in the spring of 2020, Sønderskov et al. (2020) find that the general population reacted with *reduced* psychological well-being.[11] In other words, the average Dane did not experience the improved psychological wellbeing, which we observed among our samples of unemployed individuals.

Third, our findings of positive psychological effects in response to the suspension of compliance demands are consistent with anecdotal evidence among practitioners who shared their experiences in Danish media, pointing to higher levels of mutual trust and, in response, more constructive interactions with their clients, causing the COVID-19 lockdown to be characterized as "an eye-opener—and maybe even a first step towards a new and worthy approach towards unemployed people" (Olsen 2020). In a survey of Danish job center leaders, conducted by Local Government Denmark, 94% answered, informed by their experiences during the COVID-19 crisis, that it would be helpful to the municipalities' employment efforts if mandated physical control meetings were largely replaced by less intrusive digital solutions used during the pandemic (Local Government Denmark 2020). While there are limits to how much emphasis one should put on anecdotal evidence like this, the experiences of these practitioners (see also Dragsdahl 2020) add to the face validity of our conclusions.

Throughout our investigation, we have focused on ATEs, meaning that no light has been shed on what groups of citizens are most (or least) affected by compliance demands in relation to state interactions. Recently, scholars have pointed to different kinds of human capital, such as cognitive resources (Christensen et al. 2020) and domain-specific skills (Bisgaard 2018, 2020; Döring 2021) with a potential to affect people's ability to cope with burdensome state actions. Masood and Nisar (2020) suggest that the duration and frequency of people's encounters with the state may matter as well. Over time, as people are repeatedly exposed to bureaucracy, they will gradually develop higher levels of administrative capital, defined as the "ability to understand bureaucratic rules, processes, and behaviors to achieve favorable outcomes in bureaucratic encounters" (11). A supplementary analysis (reported in supplementary appendix Q) shows no consistent evidence of our effects being moderated by unemployment duration. In Study 2, the psychological effects of the suspension of compliance demands seem to be largest among respondents who have been unemployed for a long time, which is contrary to what one should expect based on the arguments of Masood and Nisar (2020). However, in Studies 1 and 3, effects do not at all seem to be moderated by the duration of people's unemployment.[12] We encourage more

11 Andersen, Fallesen, and Bruckner (2020) find some positive psychological effects of the lockdown but *only* among parents living with children at home, meaning that, according to the authors, "living with children at home may have (…) buffered the potential mental health sequelae of the COVID-19 shutdown" (18).

12 Note, however, that our measure of unemployment duration measures the duration of people's *current* period of unemployment. In effect, we are unable to account for the potential of earlier experiences (e.g., earlier periods of unemployment causing people to have prior experience with the benefit system) affecting people's reactions to compliance demands and we encourage future research using alternative measures of prior experience with the system.

research on variations in the effects of compliance demands. If certain groups of individuals (e.g., the sick) are more affected by compliance demands than others, this is relevant knowledge when policymakers design programs targeted toward these individuals (e.g., sickness benefits) and when street-level bureaucrats meet these individuals at the frontline.

A final question concerns the external validity of our findings. Although our studies included recipients of two different kinds of government benefits, all our respondents were part of the same overall system (the Danish unemployment benefit system). It is relevant to ask whether compliance demands lead to psychological costs to a similar extent in other kinds of programs. In particular, we would expect compliance demands to matter more in programs where people already feel stigmatized from program participation, and where demands may serve to further reinforce feelings of stigma. Examples of such programs would be many of the anti-poverty programs in the United States, such as, for example, SNAP. Whether this is actually the case is a topic for future research to consider. Moreover, we encourage research across countries as cultural differences may affect people's responses to state actions like those investigated above. Finally, the scope of the policy change examined in Studies 2 and 3 in response to the COVID-19 pandemic is unusual. Hence, future research is needed to establish how large reductions are required for effects on well-being to occur.

Conclusion

By utilizing data collected right before and after a rare, exogenous shock in compliance demands, we provide evidence with high combined internal and ecological validity that the reduction of compliance demands in the context of unemployment benefit programs led to improved psychological well-being among target group members.

Our findings offer important contributions to the literature on administrative burden. In this literature, the actual compliance demands embedded in public policy and implementation are conceptually separated from the onerous experiences of those expected to meet them (e.g., Christensen et al. 2020; Herd and Moynihan 2019). Our findings speak to this distinction in that we show that demands and experiences, while conceptually separate, are empirically and causally related. This is important not least because studies using the administrative burden framework do often not include measures of experiences, essentially assuming that findings such as ours hold. And conversely, interpretatively oriented studies in the administrative burden literature have focused on elucidating meaning from experiences—including psychologically costly experiences—with onerous compliance demands without parsing the extent to which these experiences are effects of the severity of compliance demands themselves (e.g., Barnes and Henly 2018; Nisar 2018). Our findings show that the psychological costs embedded in these processes are likely empirically and causally tied to objective changes in compliance demands.

An important finding concerns the difference in effects of compliance demands across the different types of psychological costs. Across our studies, we find consistent evidence of an increased sense of autonomy among our respondents in response to decreases in compliance demands. Our evidence also points to lower levels of self-reported stress albeit evidence is less strong here. Finally, compliance demands do not seem to matter a lot to benefit recipients' experiences of stigma, although some evidence of an effect is seen in Study 1. This has important theoretical implications as it suggests that not all kinds of psychological costs are equally affected by compliance demands. Although it is possible that more permanent changes in compliance demands could lead to changes in experienced stigma over time, the findings suggest that stigma is more likely a result of target group membership, such as not being financially self-sufficient (Caswell, Larsen, and Sieling-Monas 2015; Dias and Maynard-Moody 2007) or feeling associated with negative stereotypes of government benefit recipients (Crocker 1999; Soss 2005; Stuber and Schlesinger 2006).

Our findings have key implications for policy and practice. State actors such as policymakers and implementers can benefit from attending more to the psychological costs of benefit recipients. While compliance demands can serve legitimate policy goals of ensuring (and signaling) a focus on program integrity (Keiser and Miller 2020), our findings underline the psychological costs of placing such burdens onto recipients. Stress, stigma, and autonomy loss are detrimental to people's general well-being (Deci and Ryan 2000; Wanberg 2012; Weinstein and Ryan 2011) and their ability to return to employment (Kanfer, Wanberg, and Kantrowitz 2001; Paul and Moser 2009; Vansteenkiste et al. 2005). Thus, the presumed positive incentive effects of heavy compliance demands on the motivation of unemployed people to re-enter employment may be partly or fully crowded out when people experience psychological costs from such burdens. In support of this proposition, the preliminary results from the Finnish basic income experiment show no negative effect on re-entering employment when providing unemployed citizens a basic benefit without additional demands (Kangas et al. 2019, 30). The good news is that the psychological well-being of citizens is to some extent within the control of state actors. Monitoring

experiences of psychological costs is thus key to ensuring an effective unemployment benefit system.

Relatedly, while compliance demands serve legitimate purposes and thus are unlikely to be permanently removed, there is a great potential in considering how to redesign demands to reduce psychological costs. In the Danish setting, one option would be to replace meetings with required physical attendance with online meetings to reduce travel and waiting time (Hjelmar, Pedersen, and Jensen 2021). Another option would be to introduce greater flexibility into demands. For instance, the requirement about applying for at least two jobs a week could be replaced with a requirement of about 10 applications over the course of 5 weeks.

Supplementary Material

Supplementary data are available at *Journal of Public Administration Research and Theory* online.

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Data Availability

The data underlying this article are available in Zenodo at https://dx.doi.org/10.5281/zenodo.4647211.

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